G. William Foster
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Greetings,

Last weekend I informed the Fermilab directors of my intention to step down from leadership of the Proton Driver Project at Fermilab, resign from the laboratory, and leave high-energy physics. This note is addressed to my co-workers who have put their hearts into the Proton Driver SCRF Linac project over the last years, with the shared belief that this project represents the best near term hope for U.S. High Energy Physics and for Fermilab; and to explain to a possibly wider audience my reasons for leaving HEP.

The proximate cause of my decision was the canceling of the pending CD-0 approval of the Proton Driver Project by DOE, on the grounds that it conflicts with the push for the "Fast-Track International Linear Collider" (ILC). This position apparently applies not only to the Proton Driver, but to any intermediate-scale projects which might provide any alternate or interim future for U.S. HEP at a cost significantly less than the ~$10B estimated cost of the ILC. I fear that this approach is likely to end very badly for the HEP program in the U.S.

(For those of you unfamiliar with the "CD" process in the DOE, the CD-0 is the first step towards project approval, the "Declaration of Mission Need". This carries with it no commitment to build the project, but is a necessary step to preserve even the option of a construction start in the following few years.)

A refusal to pursue any HEP alternatives to the ILC has already destroyed the accelerator-based HEP physics programs at DESY in Germany, at SLAC in California, and (to a lesser extent) at Cornell. It will be a tragedy to see this destructive course of action applied next to Fermilab, the last remaining high energy physics accelerator lab in the U.S. Unfortunately, those in the driver's seat (the DOE Office of Science and those in HEP who are advising them regarding the ILC) appear hell-bent on betting the future of the field on something that is patently technically impossible – namely the "fast track" construction start of the ILC on any time scale faster then ~2013, with a corresponding completion date any time before ~2020.

The symptoms of this illness are already apparent. In good times and bad, funding for High Energy Physics continues to fall behind those of other fields of science. The 8% increase in the president's FY07 budget for HEP pales behind the 25% increase for Basic Energy Sciences, and puts HEP last in line among all areas in the Office of Science. This 8% increase, if it survives, will make the HEP budget essentially flat since FY05. It also appears from the FY07 budget that HEP has once again lost the next $B-class construction slot, which is going to yet another light source, at BNL. (Interestingly, this light source proposal was tied with the Proton Driver for priority in DOE's "20-year Outlook"). Thus the reason for HEP's declining budget is brutally apparent: while different fields of science are proposing a range of politically and technically feasible projects, the Office of High Energy Physics is putting forth only a $10B mega-project on a technically infeasible time scale.

The problem for HEP is not a shortage of good, intermediate-scale physics programs to pursue. The physics output of medium-cost neutrino, muon, antiproton, and kaon initiatives could stack up well against the approved physics programs of the many initiatives in the $100M - $1B range being funded in other areas of science. New medium-scale initiatives would provide an excellent training ground for the next generation of particle physicists, produce world-class physics, and keep the field healthy in the U.S. A relevant example is the recently approved $B-class FAIR project at GSI in Europe. The Proton Driver Project could provide a platform for pursuing a wide range of similar initiatives, as well as providing a crucial technological stepping-stone to a U.S. Linear Collider, in the case that the US-ILC obtains the required financial and political backing to win the bid-to-host.

An honest and public analysis of the possible schedule for the ILC must be the opening point for the discussion. However despite years of trying, it has been impossible to elicit any honest public discussion of a technically defensible schedule for the ILC from the leadership of HEP. This is not an academic exercise or an empty political game, since unrealistic schedules for the ILC are being used to destroy prospects for any feasible mid-scale near term program in U.S. HEP.

For the last 12 years, the ILC construction start has always been 4 years away. In 1994, Dave Burke attempted to convince the Drell Panel that the Linear Collider could be built on a time scale to beat the LHC to the discovery of the Higgs; by the time of the "NLC Lehman Review" in 1999 the mooted construction start was 2003; and as recently as PAC 2005, Barry Barish presented a "technically limited schedule" with an ILC construction start in 2009. These claims are as technically absurd today as they ever were. This can be confirmed by a cursory glance at the timescale for the US SCRF R&D program. It is depressing that nobody claiming positions of leadership in HEP will publicly call them on this, and infuriating that these bogus time scales are being used to suppress all mid-scale HEP initiatives. The difficulty is that as long as the ILC remains only 4 years away, there will never be any time for any medium-scale initiatives. The DOE Office of Science and those advising them regarding the ILC must accept full responsibility for their irresponsible actions when the "reality distortion field" of the ILC schedule collapses and everyone realizes that there is no physics program, are almost no HEP users, and are no functional accelerator laboratories in the U.S. High Energy Physics program.

I do not claim any special standing or expertise on the political feasibility of the US-ILC. The current strategy of "give me $10B or give me death" may be the correct approach to take during the remainder of the Bush administration; or it may be suicidal. Everyone is entitled to his or her opinion on political issues. However, *technical reality must be respected in any strategic plan*. This is clearly not the case at present.

The Proton Driver main linac represents a 1.5% system test of the ILC. Those of you involved in the Proton Driver project know that we would have to work like hell to support a SCRF Proton Driver Linac groundbreaking in FY09, and it would be challenging but possible to complete the project by ~2012. This means building and testing 2 cryomodules a month - the same production rate achieved by CEBAF twelve years ago - and I know you guys could do it. This does not require massive industrialization on the scale of the ILC, but will bootstrap U.S. industrial expertise on modern SCRF technology. Such an effort will be absolutely essential for the U.S. to participate in the construction of the ILC, whether it is built in the U.S. or offshore.

A Proton Driver completed in ~2012 finishes at about the same time as the Euro-XFEL (a $1B project consisting mainly of a SCRF linac which is ~3% the size of the ILC). Experience with both machines would be invaluable to understand the cost and reliability requirements of the ILC. Thus an ILC construction start date in the range of 2013-2015 represents a reasonable goal for both Europe and the U.S. In point of fact, neither the Euro-XFEL nor the Proton Driver competes with the ILC, since they occur on different time scales. However until the ILC delivers a technically feasible schedule, including R&D and industrialization, from construction start through project completion, and this schedule is incorporated into strategic planning in U.S. HEP, I fear that it is "game over" for the Proton Driver project.

I apologize to all of you who have worked on technical aspects of the Proton Driver, that the battle for the project has been lost on the political front; and I apologize further that it has been lost to a clique of people spouting technical nonsense. No serious project schedule for the ILC has ever been presented. Technical advice on "ILC Industrialization" seems to be modeled on the SSC magnet program, which spent $500M "in industry" and never built a magnet. It is amazing to me that those in charge still refuse to acknowledge that the US-ILC needs a system

test. Serious reliability issues like cryogenic segmentation have yet to be addressed. Millions are being spent on unnecessary ILC Detector R&D, whereas after 10 years of development the ILC Main Linac does not have a Klystron that works. I did expect better.

It has been a great disappointment to me that I have been unable to convince DOE to agree that the Proton Driver Project is a reasonable option to preserve. But I also realize that my voice will not be heard, since after years of insisting that unpleasant technical realities be respected in strategic planning in HEP, I find myself branded as a troublemaker with an axe to grind. Since the starting points for progress must be an acknowledgement by the ILC planners that a realistic ILC schedule is well into the future, and that a U.S. system test is essential, future progress may in fact be quicker if I am not in the room. In any case, I have no desire to continue a debate in which one side feels free to ignore demonstrable facts.

Another problem that appears insurmountable is that there is no Proton Driver without the physics program, and there is no physics program without a strong public statement from the Director that the laboratory is serious about the project. The FNAL Directorate's new official position that we will wait until ~2010 to decide whether to build the Proton Driver (instead of the "fast track ILC") will evaporate the pool of potential users.

The future HEP program is being held hostage by a group of people who want to claim that there is nothing useful to do with the U.S. HEP program unless we get a $10B project, right away, no matter how technically infeasible this is. The *last thing that these people want* is a healthy, diverse U.S. HEP program that (to their minds) weakens the case for the US-ILC. Unsurprisingly, most of these people have little or no personal stake in the survival of the existing accelerator-based U.S. HEP program, (i.e. Fermilab). They are perfectly willing to play Russian Roulette with five loaded chambers, as long as they get to point the revolver at someone else's head. The hypocrisy of hearing this point of view from HEP labs like DESY and SLAC, who have carefully crafted their off-ramps from the HEP programs in the case the ILC folds, is particularly hard for me to bear politely.

This is especially galling since one needs no new data to convince oneself that the "fast-track US-ILC" does not exist technically, legally or financially. A quick look at the projected milestones of the US-ILC SCRF program should convince anybody that the US industrialization cannot be completed for at least 7-8 years. An examination of the environmental permitting requirements for a large off-site construction project yields a similar conclusion. Financially, a safe lower limit of ~$8B for the US cost of the ILC project (which can easily be obtained from a number of sources including the TESLA TDR), should be enough to convince anyone with a knowledge of recent funding of large international science projects that the ILC project is a decade away.

Instead, US HEP and Fermilab are being forced into a position where there is to be no alternative program to the ILC. Within a few years, Fermilab is to be a $300M laboratory doing $150M of ILC R&D, an accelerator division operating a JPARC-scale synchrotron, and a limited number of neutrino physicist users. Then, in the likely occurrence that we do not immediately get ~$10B of federal and international funds to build the ILC, congress may tire of funding R&D for a project with an uncertain time scale, and the HEP budget at both US universities and labs will collapse. (By this time, of course, Rep. Hastert will have retired as Speaker of the House and we will no longer have him to defend our budget….)

I am often accused of being "anti-ILC" which I believe is unfair. I have no objection to the ILC program as long as it realistically pursued and the base program protected. I had several friends in the fusion program when its budget was cut by a factor of ~2/3 ten years ago, (a cut which affected not only laboratories, but most university programs). I really do not want to be around when something similar happens to my friends in HEP.

Nonetheless, I remain hopeful that the underlying logic of the Proton Driver Linac project will ultimately prevail:

- It makes no sense to discuss a US-ILC without a %-scale main linac system test,
- It makes no sense to build the US-ILC systems test anywhere but Fermilab,
- It makes no sense to site the test linac where it *cannot* be used for a Proton Driver, as well as for the many other uses a general-purpose SCRF linac could serve.

All of these imply that a complete technical design for the SCRF Proton Driver, including transfer line, injection, shielding, and environmental permitting, is needed well before linac groundbreaking. Those of you familiar with large project schedules know that this process must begin several years before groundbreaking, to provide even the option of proceeding with project construction. This was the plan had a CD-0 been granted, but cannot be the plan now that the CD-0 has been denied.

I am also hopeful that the Proton Driver "R&D" program will be well supported, even as the Proton Driver "Project" is sacrificed on the altar of the "Fast-Track ILC". Building a 100 MeV SCRF front-end linac driven by a single Klystron, controlled by fast-ferrite phase shifters, is world-leading R&D that will change forever how these machines are built. There is great interest internationally in the development of "ILC-Compatible" SCRF spoke resonators and elliptical-cell cavities for these front-end linacs. These could represent a real "spin-off" from the ILC. The concept of a "multi-species" linac with a TESLA-style RF fanout controlled by fast-ferrite phase shifters has attracted interest and emulation worldwide. The long-pulse SCRF option being pursued for the Proton Driver, when successful, could save hundreds of millions of dollars for the ILC. This is a rare chance for Fermilab to do important world-leading accelerator R&D, even if this real R&D program does not appear to be well aligned to the imaginary axis of the fast-track ILC schedule.

It is truly a shame that our steamship seems to have a committee of captains who believe that the ductility of carbon steel rivets at freezing temperatures is just a political problem. However, technical reality still has some real friends inside the US-ILC effort: Sergei Nagaitsev and Bob Kephart don't seem to be shy about tolerating technical BS. The gang-of-four (Bob Webber, Giorgio Apollinari, Petr Ostroumov, and Jim Kerby) who have served as the next level of management in the Proton Driver Project are as good as it gets, and are fully capable of carrying on the R&D project. Members of the multi-lab collaboration which was formed to pursue the Proton Driver Project collaboration are also top-notch. The Proton Driver Design Study provides a workable and rather complete blueprint for the project. So, there is still reason for hope.

I'd like to close by noting one of the amusing criticisms that I've been subjected to over the last few years, namely: "how come Foster has all of the good guys working for him on the Proton Driver, instead of forcing them to work on the ILC?" This is not an accident, of course: you guys were my top draft choices. I selected you carefully and recruited you hard. I was able to convince you to work on SCRF because you felt the Proton Driver was an interesting and "technically real" project, and a legitimate stepping stone to the eventual goal of the ILC. I hope that you are allowed to carry that enthusiasm through the Proton Driver effort as it continues as an R&D program, and I truly hope that it re-emerges as something that provides an important contribution to a bright future for FNAL's accelerator-based HEP program.

and keep smiling,

- Bill Foster
Feb. 19, 2006